CONCENTRATED POVERTY AND COLLEGE OUTCOMES: A MATCHED PAIRS ANALYSIS OF NEIGHBORHOOD EFFECTS

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ABSTRACT

Brian Louis Levy: Concentrated Poverty and College Outcomes:
A Matched Pairs Analysis of Neighborhood Effects
(Under the direction of Anthony Daniel Perez)

Does neighborhood disadvantage affect an individual's odds of matriculating at and graduating from college? Research on postsecondary outcomes tends to be non-experimental in nature and finds diminished neighborhood effects when compared to research on secondary outcomes. The limited experimental research focuses only on matriculation and yields contradictory findings. This research uses propensity score matching to account for neighborhood endogeneity and analyze the impact of residing in concentrated poverty during adolescence on college outcomes. The research also assesses institutional, collective socialization, relative deprivation, and epidemic models as mediators for neighborhood effects. Results suggest that concentrated poverty negatively impacts college outcomes, with the strongest effect on college graduation. The mechanisms by which neighborhoods affect collegiate outcomes differ over the life course. The strong, negative impact of neighborhood poverty on college graduation is explained by neighborhood economic opportunity, offering support for collective socialization theory.

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LIST OF ABBREVIATIONS

GPA	Grade point average
MTO	Moving to Opportunity
PVT	Picture Vocabulary Test

CONCENTRATED POVERTY AND COLLEGE OUTCOMES

Substantial research demonstrates the sizeable economic returns to a college degree (Barrow and Rouse 2005; Card 1999), and antipoverty policymakers and researchers often tout education as a key component to improving low-income individuals' life chances (Haskins 2011; Jacob and Ludwig 2009). Although it may be an economically rational choice, pursuing higher education is determined by more than just individual factors, as growing up in a disadvantaged community is associated with lower educational attainment (Garner and Raudenbush 1999; Harding 2003; Wodtke et al 2011). The relationship between growing up in concentrated poverty and educational trajectories is especially important given President Obama's recent call in the State of the Union to build "ladders of opportunity" out of such "pockets of poverty" (The White House 2013).

Scant research has examined the relationship between neighborhood disadvantage and college matriculation and completion, with most research on the effect of neighborhood disadvantage focusing on primary or secondary educational outcomes. Research on postsecondary outcomes tends to be non-experimental in nature and finds diminished neighborhood effects when compared to research on secondary outcomes (Harding 2011; South et al 2003; Vartanian and Gleason 1999). Experimental analyses (Orr et al 2003; Rosenbaum 1991) and research using sibling pairs (Aaronson 1998; Plotnick and Hoffman 1999) that attempt to identify causal estimates of neighborhood effects on ever attending college yield contradictory findings. Experimental and quasi-

experimental analyses of college completion – the outcome most salient for future life chances – are notably absent from the literature.

Following previous research suggesting there may be especially detrimental impacts of highly disadvantaged communities (Crane 1991; Brooks-Gunn et al 1993) and recent calls for investigation of non-linearity in the effect of neighborhood disadvantage (Burdick-Will et al 2011), I use matched pairs to examine the impact of concentrated poverty (a neighborhood poverty rate above 40%) on college matriculation and completion. Two reasons previous research may fail to detect a relationship (or detect an attenuated relationship) between neighborhood disadvantage and collegiate outcomes are that there may be critical thresholds of disadvantage and the mechanisms by which neighborhoods affect educational outcomes may change over the life course. This research uses the National Longitudinal Study of Adolescent Health (Add Health) – a longitudinal data set providing rich individual and contextual data (including the unique ability to control for school effects[1], as well as peer characteristics) – to examine how neighborhood disadvantage matters for college matriculation and completion and why previous research may not have found such a relationship. Further, given the research demonstrating the highly-localized nature of poverty saturation (Lichter et al 2008; Lichter and Johnson 2007), I operationalize neighborhoods at the block-group level, which is a lower level of analysis than previous research using census tracts.

This research contributes to the neighborhood effects literature by examining the relationship between concentrated poverty and an under-examined but substantively important predictor of life chances – college graduation (as well as matriculation). The

[1] Add Health offers the ability to control for school effects by matching within communities.

analysis uses propensity score matching to minimize selection effects and estimate relationships that may approximate true causal associations better than traditional regression techniques. Findings assess collective socialization, institutional, relative deprivation, and epidemic models as mediators for the focal relationship. Results suggest that concentrated poverty negatively impacts college graduation, and confirming previous research, the most-harmful effects of neighborhood disadvantage occur several years after residence in a distressed community. The negative impact of neighborhood poverty on graduation odds is explained by neighborhood economic opportunity, offering support for collective socialization theory. Any relationship between neighborhood disadvantage and college matriculation seems to be the result of either adolescents' educational achievement/expectations or neighborhood economic opportunity, indicating that there may be a different mechanism for neighborhood effects on short-term outcomes. Epidemic and institutional theories have less support in the results, and relative deprivation is unsupported.

Theoretical Perspectives

Wilson's (1987) seminal essays on the negative effects of neighborhood disadvantage initiated an abundance of new research on the topic that persists today. Disadvantaged neighborhoods can affect individuals' outcomes through a variety of mechanisms. Jencks and Mayer (1990) note the potential harmful effects of disadvantaged neighborhoods through collective socialization, institutional, relative deprivation, and epidemic models; these models organize the present review of the manner in which neighborhood disadvantage affects individual outcomes.

Collective socialization models posit that neighborhood adults, serving as role models and evidence of local life chances, transmit neighborhood advantage. Greater numbers of adults modeling work behavior and achieving economic success raises the value of these behaviors for youth, who are then more likely to work hard with the belief that success is possible (Jencks and Mayer 1990). Wilson (1987) discusses collective socialization mechanisms for transmitting disadvantage extensively. He argues that the exodus of the black working class and middle class professionals from central cities in the late 1960s and 1970s, coupled with declining job opportunities, left an underclass that was increasingly isolated. Effects of this isolation included a decline in public institutions, increasing social disorganization, and growing prevalence of single-parent families. Clear within this framework are arguments about economic opportunities, neighborhood stability, and social control. Duncan (1994) applies this context to education and finds that affluent neighbors are related positively to years of completed schooling for white and black youth, although black male youth may only benefit from affluent black male neighbors. He also notes that social integration is another form of collective neighborhood socialization, and racial integration is associated positively with years of completed schooling for black males (Duncan 1994).

Sampson (2011) highlights two neighborhood processes as examples of collective socialization: 1) norms and collective efficacy; and 2) social networks and interactions. Norms are the shared understandings and expectations of life chances within a neighborhood. Neighborhoods characterized by trust, interaction, and willingness to intervene on behalf of other neighborhood residents develop a sense of collective efficacy and are likely to transmit the advantages of the neighborhood to its residents. At the

individual level, neighborhoods offer their residents a host of individuals with which they can interact. The social networks that individuals form and the interactions they yield provide social capital for individuals. This social capital, of course, is highly variable by neighborhood; for example, individuals in advantaged neighborhoods are more likely to have neighbors that are college graduates and may provide knowledge of and assistance regarding the college application, matriculation, and completion process.

Research examining neighborhood effects on educational outcomes yields substantial support to the collective socialization model, with both the social/economic capital and neighborhood norms/efficacy processes as potential explanations. First, a disadvantaged neighborhood offers less social and economic capital for its residents to apply toward the college experience. Ainsworth (2002) and Brooks-Gunn et al (1993) find that the share of high-status workers, the occupational expectations of the community, and the share of adults with a college degree are related to educational achievement and attainment. Neighborhoods with low social and economic capital could experience diminished postsecondary attainment because of financial difficulties and challenges navigating the college experience. Social integration may be the primary predictor of college dropout (Charles et al 2009), and perhaps the social networks formed during youth structure the types of networks and levels of integration later in life. In addition, low socioeconomic status is correlated with nontraditional academic pathways characterized by interrupted college attendance, and financial strain is a plausible mediator of this relationship (Goldrick-Rab 2006).

Second, to the extent that neighborhood disadvantage affects internalized norms and collective efficacy, individuals might be more likely to express antisocial behavior and

exhibit diminished agency and life expectations. Although I am unaware of research exploring the significance of neighborhood norms for individual collegiate outcomes, research demonstrates the importance of other types of norms. Parental engagement in the secondary educational process and peer norms of academic behavior/expectations are established correlates of postsecondary enrollment (Horn and Chen 1998). Research paints a clearer portrait regarding the impact of neighborhood efficacy. Neighborhood racial/ethnic integration is positively related to years of schooling for black youth (Duncan 1994), especially when experienced during adolescence (Halpern-Felsher et al 1997). Charles et al (2009) find that although residential segregation – a correlate of concentrated disadvantage – does not directly affect college grade point average (GPA) after two years, it does affect stress levels and individual cognition through exposure to disorder and violence; stress and cognition are in turn related to GPA. Social control, though not quite as substantive as other forms of collective socialization, also may mediate neighborhood effects (Ainsworth 2002).

Institutional models contend that institutions link community members together, and the quality of neighborhood institutions affects community interactions and community members' outcomes (Sampson 2011). An example of how institutions may differ across neighborhoods would be schools in advantaged neighborhoods being better able to attract and retain superior teachers than schools in disadvantaged neighborhoods (Jencks and Mayer 1990). Research on disadvantaged neighborhoods suggests a lack of access to social service organizations (Allard 2008), but organizations in disadvantaged neighborhoods may counter the lack of neighborhood resources by using alternative strategies like forming more ties to other organizations (Small et al 2008). Applied to

education, this perspective might suggest that students in disadvantaged neighborhoods have lower access to superior teachings and high-quality learning opportunities, but schools may counter these deficits using alternative strategies that are not as resource intensive. Ainsworth (2002) finds some support for institutional mediating, but on the whole institutions – especially schools – are under-examined mediators in the neighborhood effects literature (Jencks and Mayer 1990; Leventhal and Brooks-Gunn 2000).

Relative deprivation models are based on the notion that individuals judge their wellbeing relative to that of other individuals. Thus, a child growing up in an advantaged neighborhood has a greater number of advantaged peers, who are more likely to be successful and decrease the child's evaluation of his or her success (Jencks and Mayer 1990). In practice, relative deprivation theory might suggest that a student with a GPA of roughly a B attending a high school where the mean GPA is an A would be less likely to apply for college than a comparable student in a high school with a C average. Moreover, students in relatively high-achieving schools may find the competition for grades to be more difficult than students in low-achieving schools. In support of the relative advantage/deprivation model, Crowder and South (2011) find that advantage of nearby neighborhoods reduces the odds of graduating high school for white students.

Epidemic models hypothesize that peer behavior affects individuals, and a neighborhood concentrated with disadvantage is likely to have a greater share of individuals behaving in an antisocial or non-normative manner. Thus, individuals growing up in these neighborhoods are more likely to 'catch' the negative behavior contagion (Jencks and Mayer 1990). Crane (1991) further develops this theory by arguing that the

impact of neighborhood disadvantage is non-linear, producing particularly high rates of negative outcomes at the highest concentrations of disadvantage. He finds a clear, non-linear increase in black and white teenagers' odds of dropping out of high school as the percentage of workers employed in managerial and professional jobs decreases. Results are strongest for males; teenage girls demonstrate increased odds of teenage pregnancy in severely distressed neighborhoods, suggesting that neighborhood disadvantage may affect different outcomes for males and females. These associations are most substantive in the largest cities, indicating that high-volume concentration of neighborhood disadvantage is particularly problematic.

Research suggests that both peer effects and threshold effects may mediate the relationship between neighborhood disadvantage and educational outcomes. South and coauthors (2003) find that the relationship between neighborhood economic success/resources and educational attainment is mediated mostly by the educational performance of adolescents' peers. Harding (2011) similarly reports cultural heterogeneity in schooling attitudes may account for a significant portion of the relationship between neighborhood economic disadvantage and college enrollment. Potential threshold effects may be obscured by the operationalization of disadvantage. Whereas early research did not find a detriment to living in low-income neighborhoods relative to moderate-income neighborhoods[2] (Brooks-Gunn et al 1993; Duncan 1994; Duncan et al 1997; Halpern-Felsher et al 1997), there is suggestive evidence of a threshold effect and a negative impact of concentrated poverty (Brooks-Gunn et al 1993). In a recent reanalysis of Moving to Opportunity (MTO) data disaggregated by site, Burdick-Will et al

[2] High income neighborhoods were beneficial.

(2011) find evidence of nonlinear (threshold) effects of concentrated disadvantage and violent crime on youths' educational outcomes.

Finally, it is worth noting that individual processes and demographics can mediate or moderate the impact of neighborhood disadvantage. Among individual processes, research suggests that educational expectations (Ainsworth 2002; Jacob and Wilder 2011) and aspirations (South et al 2003) may be particularly important mediators. One reason individual attitudes toward education may mediate neighborhood effects is that the poor economic prospects and low social cohesion of disadvantaged neighborhoods call into question the utility of higher education and lead adolescents to reduce expectations and/or aspirations (MacLeod 1996, as cited in South et al 2003; Stewart et al 2007) – although some research questions the magnitude of any effects (Nichols et al 2010). Since individuals update their expectations at least once between 8th grade and eight years later (Jacob and Wilder 2011), adolescence is likely a critical period for any neighborhood effect on expectations and aspirations. Educational risk behaviors (e.g., truancy, suspension, course failure, etc.) and student-reported parent and teacher support do not moderate neighborhood effects (Connell and Halpern-Felsher 1997).

Among individual demographics and family characteristics, established moderators of neighborhood effects include race (Crowder and South 2011; Crowder and South 2003; Halpern-Felsher et al 1997; Vartanian and Gleason 1999), gender (Connell and Halpern-Felsher 1997; Crowder and South 2011; Crowder and South 2003; Entwisle et al 1994; Halpern-Felsher et al 1997), family income (Wodtke et al 2012), and family structure (Crowder and South 2003). Family income and race may be of increasing salience. Income is now nearly as strong a predictor of student achievement as parental education (Reardon

2011), and black youth's overrepresentation in the most-disadvantaged neighborhoods has grown stronger over time (Crowder and South 2003).

Estimating Neighborhood Effects on Educational Attainment

A wealth of empirical research evaluates the role of neighborhoods in educational attainment. Most of this research focuses on pre-collegiate outcomes, which I review below because the associations are plausibly similar to the relationship between neighborhood effects and collegiate outcomes. The research focusing on collegiate outcomes tends to focus on ever attending college (as opposed to graduating) and is not experimental or quasi-experimental in nature.

Non-experimental research has established a relationship between neighborhood context and several pre-college educational outcomes: academic achievement (Ainsworth 2002; Brooks-Gunn et al 1993; Duncan et al 1997; Entwisle et al 1994; Sampson et al 2008; Sharkey and Elwert 2011), dropping out of high school (Brooks-Gunn et al 1993; Crane 1991; Crowder and South 2003; Ensminger et al 1996; South et al 2003), graduating from high school (Crowder and South 2011; South et al 2003), and years of completed schooling and educational attainment (Ensminger et al 1996; Garner and Raudenbush 1991; Halpern-Felsher et al 1997).

Less research examines postsecondary outcomes, especially college graduation. Non-experimental analyses demonstrate negative associations of neighborhood disadvantage with both attending college (Harding 2011; South et al 2003) and graduating from college (Vartanian and Gleason 1999). These associations are typically smaller in magnitude than estimated relationships with high school outcomes (South et al 2003). They also vary by race; neighborhood disadvantage is related negatively to college

graduation of white youth but not black youth (Vartanian and Gleason 1999).[3] Research using sibling pairs that vary in age by at least three years in an attempt to control for endogeneity yields inconsistent findings. Aaronson (1998) finds a significant relationship between neighborhood disadvantage and college enrollment, but the estimated association is smaller in magnitude than neighborhood effects for secondary educational outcomes. In contrast, Plotnick and Hoffman (1999) suggest the relationship may be spurious and resultant from selection effects.

The endogeneity of neighborhoods is so critical that Jencks and Mayer (1990, p. 119) recognize the difficulty in distinguishing neighborhood effects from family selection effects as "perhaps the most fundamental problem confronting anyone who wants to estimate [neighborhood] effects." One technique researchers use to account for selection effects is matched pairs. Aaronson (1998) and Plotnick and Hoffman (1999) employ such a technique with their analysis using sibling pairs and family fixed effects. Although this method advances this literature on neighborhood selection, it is limited by the assumption that family selection factors do not change over time; to estimate the model, siblings must differ in age by several years and not reside in the same neighborhood at the age of analysis. Along with this strong assumption, such analyses also disallow analysis of non-moving families and only children. Other researchers are using propensity matching to overcome these limitations and analyze a range of social outcomes (Caliendo and Kopeinig 2005). Harding (2003) has applied this technique to study youth's odds of dropping out of high school, but researchers have yet to use propensity matching to analyze the relationship between concentrated poverty and college matriculation and completion.

[3] The authors also find that neighborhood disadvantage is associated with increased odds of dropping out of high school for black youth but not white youth.

Like all non-experimental and quasi-experimental techniques, however, propensity matching makes several strong assumptions. First and foremost, the technique assumes that all variables affecting collegiate outcomes that also affect residence in concentrated poverty are either included in the matching equation or highly correlated with variables in the matching equation. To the extent that this is not the case, estimates of the focal relationship will be upwardly biased. A seminal paper by Lalonde (1986) highlights the extent to which nonexperimental designs can yield biased results even after controlling for all observed confounders. Although follow-up research (Dehejia and Wahba 1999) suggests that propensity matching may provide estimated treatment effects that are much closer to experimental estimates than LaLonde's nonexperimental estimators, Dehejia and Wahba's findings are sensitive to their subsample of LaLonde's data and their selection of matching variables (Smith and Todd 2005). Ultimately, even if propensity matching offers quasi-causal estimates that are less sensitive to misspecification than traditional regression models (Rubin et al 2004), knowledge and measurement of the important selection variables remains critical. Without this, propensity matching will succumb to bias like all nonexperimental techniques. A second assumption of propensity matching is sufficient overlap between the treatment and control populations to ensure that the final set of matched pairs are balanced along the selection dimensions. If the assumptions of matching are met, any differences between the treatment and control populations after the treatment can plausibly be attributed to the treatment.

Two social programs provide opportunities to control for selection effects without making these assumptions by establishing experimental designs. First, the Gautreaux was administered by Chicago's nonprofit Leadership Council for Metropolitan Open

Communities and established in response to a Supreme Court decision on housing desegregation. The quasi-experimental program offered public housing residents Section 8 vouchers to move to private apartments, and the Leadership Council assigned movers to a neighborhood based on housing availability, giving a sense of randomizing the assignment of families to neighborhoods (Rosenbaum 1991). Participants, however, did have some choice in their neighborhood assignment, and this choice presents the opportunity for individuals to differentially select into their new neighborhoods based on a host of background characteristics.

Gautreaux's results are indicative of neighborhood effects. Children moving to relatively advantaged locations and remaining in the program for at least 7 years achieved at equivalent or higher levels and were much more likely to attend college than children moving to relatively disadvantaged locations. Movers do not differ in socioeconomic characteristics by location – although children moving to the suburbs were more likely to be male – but it is possible for movers to vary on psychosocial dimensions like willingness to move to a suburban, predominantly white neighborhood (Rosenbaum 1991).

Second, the MTO demonstration classifies as an experiment through its use of the gold-standard randomized control trial research design. MTO randomly assigned participants to either receive assistance in moving out of subsidized housing in highly impoverished neighborhoods of five large cities (treatment group) or not receive assistance but still remain eligible for government services (control group). Assistance recipients were divided into two groups: one received housing vouchers for neighborhoods with poverty rates under 10 percent, as well as counseling in finding and leasing a unit, and

the other received Section 8 vouchers with no location restrictions and no relocation counseling (Orr et al 2003).

Four to seven years after treatment, MTO treatment children were attending schools of somewhat greater quality with fewer poor students than control children, but quality improvements were not substantial. The small change in school quality may be due in part to reluctance of parents to move their children to – or desire to keep them enrolled in schools located in – their new neighborhood. Furthermore, MTO is not associated with significant changes in academic achievement, educational progress, or college attendance (Orr et al 2003; Sanbonmatsu et al 2006), although some research finds positive effects for certain sub-populations. Kling et al (2007) document improved educational outcomes for female youth ages 15-20, but the authors do not find any effects for younger girls, suggesting that neighborhood effects on education may be the strongest in adolescence. Ten to fifteen years after treatment, the null findings on educational outcomes largely persist. MTO is not associated with any change in college matriculation, and the program may have a small negative effect on expectations of enrolling in college (Sanbonmatsu et al 2011).

Researchers advance several hypotheses for why MTO did not yield large-scale improvements in educational outcomes. Foremost among these is that, despite its excellent causal design, MTO failed to produce substantive changes in overall neighborhood disadvantage. The substantial reductions in neighborhood disadvantage expected under MTO did not materialize, especially over the long-term. Although the program sought to facilitate moves out of racially-segregated and impoverished neighborhoods, most of the movers in MTO actually moved into racially segregated neighborhoods. In addition, many

compliers who moved into low-poverty neighborhoods eventually relocated into high-poverty neighborhoods (Clampet-Lundquist and Massey 2008).

Sampson (2008) uses the example of Chicago to further unpack the small changes in neighborhood conditions. Six to seven years after MTO, neighborhood poverty rates (at the tract level) for all groups remained above 30 percent; even for the compliers in the experimental group, neighborhood poverty remained above 20 percent – itself an accepted threshold for high poverty. A host of other measures of neighborhood disadvantage (e.g., social cohesion, social control, organizational participation, violence, disorder) were unchanged. Along with the small differences in neighborhood conditions between experimental and control groups, neighborhood trajectory was actually superior for the control group. During the study period, neighborhood disadvantage was declining in Chicago, but the rate of decline was quicker in neighborhoods inhabited by control group members. Finally, even when individuals (including compliers) did use the MTO experiment to move to a new neighborhood, they rarely moved far from their original neighborhood, and many individuals eventually moved back into their original neighborhood.

Along with the modest reductions in neighborhood disadvantage, other reasons MTO may not have affected educational outcomes include: non-compliance among the experimental group; persistence of children in disadvantaged schools; destruction of social ties; and operationalization of neighborhood poverty at the wrong level of geography (census tract as opposed to block group). Clampet-Lundquist and Massey (2008) observe that of those assigned to the experimental group, only 47 percent actually used their vouchers to change neighborhoods. Moreover, as noted above, few of these movers

persisted in their new neighborhoods. Sanbonmatsu et al (2006) suspect that the scant improvement in school quality, as well as the large number of students persisting in their previous schools, drives the insignificant educational outcomes for treatment group youth. Small et al (2008) note that moving individuals to new, low-poverty neighborhoods could have taken them away from well-connected social service organizations from which they may have received benefits. Burdick-Will et al (2011) suggest that improvements in neighborhoods are only beneficial for families who want to live in the less-disadvantaged neighborhoods. Finally, Hipp (2007) finds that whereas tract-level economic resources are unrelated to neighborhood social disorder and violence, block-level economic resources are related to such outcomes.

Ultimately, a key lesson of MTO may be that it is not possible to analyze the full effects of comprehensive, severe neighborhood disadvantage using an experimental design. Creating significant, lasting changes in neighborhood quality for a large share of the experimental group is extremely difficult, and the disruptive effects of a move, such as breaking beneficial social ties, could offset beneficial neighborhood changes. Instead, there is an important role for observational research in establishing contextual effects (Sampson 2008). Propensity score matching is an observational technique that attempts to create quasi-experimental conditions by balancing an analytic sample of pairs on known predictors of the focal variable, and Harding (2003) has applied this technique to analyzing the impact of neighborhood poverty on secondary educational attainment.

Again, an important consideration for propensity matching is developing a robust, exhaustive model of selection into the treatment. Harding (2003) discusses selection into neighborhood disadvantage extensively, and I adapt his selection model for my analysis.

To be sure, the assumption of a well-specified selection model can always be questioned, but by incorporating all known correlates and confounders in the selection model, potential bias can be minimized. In research on stratification in neighborhood attainment, Sampson and Sharkey (2008) find that race, ethnicity, income, and education are the key sources of attainment stratification; other psychosocial factors like depression, social support, and criminality do not contribute substantively. Quillian (2003) also notes the importance of living in a female-headed household, along with race, for odds of living in a high-poverty neighborhood (>20% poverty). This provides reassurance that my selection model (discussed later), which contains these key variables and a host of other theoretically important variables, is not subject to substantial bias.

Concentrated Poverty as a Critical Threshold for Social Outcomes

Propensity matching requires a binary treatment variable, which can be undesirable due to the loss of information in reducing a continuous variable to a dummy variable. However, the literature suggests that concentrated poverty may be a key threshold for the effect of neighborhood disadvantage, and Burdick-Will et al (2011) call for new research investigating the potential non-linear nature of neighborhood effects. I briefly review the literature on concentrated poverty here to demonstrate its potential to be especially detrimental for individual life chances.

Concentrated poverty is a "standard yardstick for high poverty status in studies of urban poverty" (Small et al 2008, p. 402). Research demonstrates that residing concentrated poverty is an important determinant of life chances and individual wellbeing, burdening individuals in a variety of ways beyond the challenges already imposed by their economic circumstances. Researchers at the Federal Reserve System and the Brookings

Institution (2008) review the literature on concentrated poverty and note a variety of negative economic outcomes: low school quality and academic achievement; employment discrimination and reduced job networks; devaluation of homes; and challenges amassing wealth. The researchers also highlight a number of non-economic costs: increased crime risk, especially violent crime risk (see also Bjerk 2010); poor health; and lower-quality health care (The Federal Reserve System and the Brookings Institution 2008). For low-income individuals living in poverty-saturated areas, social service organizations tend to be less proximal, and distance from these organizations is an important determinant of service utilization (Kissane 2010). By contrast, low-income individuals living in relatively-advantaged areas tend to be "buffered from the most negative impacts of poverty" (Dwyer 2010).

Concentrated poverty also limits the economic potential and social cohesion of the neighborhood and broader community through associated residential segregation. Minorities, especially black individuals, tend to be most disadvantaged in the residential sorting process (Crowder and South 2008; Sampson and Sharkey 2008), and wealth barely explains the gaps in neighborhood attainment (Crowder et al 2006). Residential segregation by race and ethnicity, which is related to concentrated poverty (Massey and Denton 1993; Quillian 2012), is an important determinant of the greater rates of spatial mismatch from jobs experienced by racial and ethnic minorities (Stoll and Covington 2010). Individuals residing in concentrated poverty – especially Latinos, who are most likely to use personal, neighborhood contacts in searching for a job – are less likely to use neighborhood contacts during job search (Elliot and Sims 2001).

Given the challenges of poverty saturation, it is unsurprising that children growing up in high-poverty communities (>20% poverty) are 50 percent more likely that children growing up in communities of less than 10 percent poverty to be economically worse off than their parents (Sharkey 2009). Unfortunately, more children are growing up in distressed communities. The number of children living in severely-distressed neighborhoods (>20% poverty with other human capital challenges) increased by nearly 1 million (19 percent) in the 1990s (O'Hare and Mather 2003). This suggests that concentrated poverty will be an important dimension of stratification well into the future.

Data

Add Health (Harris 2009) provides excellent data for assessing the effect of growing up in concentrated poverty on collegiate outcomes for today's youth. Add Health is a longitudinal, nationally representative study of U.S. high schools and adolescents in grades 7-12 during the 1994-1995 school year. Add Health used a clustered sampling strategy and gathered data from one high school and one feeder middle school in 80 communities across the United States (Harris et al 2009). The clustering of students within schools permits me to control for school effects by matching students within community; this differs from most research on neighborhood effects, which rarely adjusts for school effects (Jencks and Mayer 1990; Leventhal and Brooks-Gunn 2000). Add Health also used addresses and GPS location to attach substantial contextual data from the 1990 census to each in-home interviewee; the richness of these data is beneficial for my research.

Along with in-school interviews of approximately 90,000 students at Wave I, Add Health interviewed a core sample of roughly 200 students per school pair and a number of special oversamples (e.g., ethnic, genetic, disability) in their homes as well. The in-home

sample of 20,745 adolescents constitutes the longitudinal Add Health cohort that is followed through time. Researchers have collected four waves of data to date: Wave I from 1994-1995; Wave II re-interviewed in-home respondents who were not seniors in 1996; Wave III re-interviewed in-home respondents in 2001-2002; and Wave IV re-interviewed in-home respondents in 2008-2009. Sample attrition is well within accepted levels; for instance, the Wave IV follow-up of Wave I respondents had an 80.3 percent response rate (Harris 2011).

I use data from all four waves of Add Health for this research. Because predicting propensity scores requires temporally-prior information, the Wave II in-home sample (n=14,724) is the starting point of my analytic sample, and I drop observations (n=209) without contextual data on concentrated poverty. Due to the number of survey waves in Add Health prior to individuals becoming college aged, as well as the computational requirements of my identification strategy, I observe neighborhood disadvantage only once (during adolescence) in my analysis. This is potentially problematic as point-in-time measures of disadvantage understate the relationship between neighborhood effects and educational attainment (Wodtke et al 2011; Wodtke et al 2012), and neighborhood disadvantage across multiple generations may even be related negatively to children's cognitive abilities (Sharkey and Elwert 2011). Nevertheless, research suggests observation once during adolescence is a reasonable option and likely to produce effects that are only somewhat downwardly biased (Crowder and South 2011). Wodtke et al (2012) find that neighborhood disadvantage in childhood is unrelated to high school graduation, whereas disadvantage during adolescence is negatively related to graduation. Moreover, the impact of disadvantage is strongest several years following residence in a disadvantaged

neighborhood (Sampson et al 2008). Combining these findings and applying them to adolescents suggests that neighborhood disadvantage should have a particularly strong impact on college matriculation and completion, in contrast to the understated relationships observed in previous research. Below, I describe my matching variables that control for selection effects and are used to develop matched controls for individuals residing in concentrated poverty at Wave II. I then describe the dependent and independent variables for my analysis.

Matching variables include Wave I adolescent, parental, and household characteristics, as well as reasons for living in Wave I neighborhood. Adolescent race is the five-category measure constructed by Harris et al (2009) that includes non-Hispanic white, non-Hispanic black, non-Hispanic Asian, non-Hispanic Native American or other, and Hispanic. Immigrant generation is the three-generation identification used by Harris and coauthors. Other adolescent selection variables are sex (female=1) and low birth weight status. Parental selection variables include a five-category highest parental education measure developed by Harris and Ryan (2004), disability status, nativity status, and parental age at adolescent's birth. Household selection variables are a four-category measure of family structure (Harris and Ryan 2004), household size, household income, public assistance receipt, and whether or not the home language is English. Finally, parent-reported reason(s) for residing in Wave I neighborhood and primary reason for residence are selection variables. Tables 1a and 1b summarize the matching variables.

College matriculation and completion are the outcome variables and are measured at Wave IV when the adolescents are age 24-32. College matriculation includes enrollment in a community (2-year) college or 4-year postsecondary institution, but it does not include

participation in job training or a vocational program. College graduation includes completion of a bachelor's degree only because of the substantial economic returns to a 4-year degree even when compared to a 2-year degree. Individuals missing data on collegiate outcomes at Wave 4 but reporting having enrolled or graduated in Wave 3 are coded as enrollees or graduates, respectively; otherwise, their data remain missing.

Contextual predictors of the postsecondary outcomes include the focal variable, residence in concentrated poverty, and several mediating mechanisms within the collective socialization, relative deprivation, and epidemic models. The level of analysis of neighborhood characteristics is likely important. Measuring concentrated poverty at the county, census tract, and block group levels yields strikingly different compositions and distributions of populations residing in concentrated poverty. At the census tract level, metropolitan concentrated poverty appears clustered in a small few central city communities – a segregated population (Jargowsky 2003). Block-level analysis, however, finds that three-quarters of high-poverty blocks (>20% poverty) are located in low-poverty counties (Lichter et al 2008). Lichter and Johnson (2007) note that "we cannot discount the possibility – even likelihood – that the geographic scale of significant rural [concentrated poverty] has simply been redefined to the micro-scale level or that aggregate county patterns mask patterns for other important population subgroups."

I account for the geographic-sensitivity of concentrated poverty by analyzing it and other neighborhood characteristics at the block group level. Specifically, I define residence in a neighborhood of concentrated poverty as living in a block group with a poverty rate greater than 40 percent as measured by the 1990 census. This yields a sample of 987

adolescents residing in concentrated poverty and still in high school at Wave II. All contextual covariates are measured at the block group level unless otherwise specified.

Collective socialization measures include two indexes of social disadvantage (economic opportunity and resource deprivation) and several neighborhood stability variables. The social disadvantage indexes are generated by principle component factor analysis (rotated) of the standardized[4] component variables (factor loadings on economic opportunity and resource deprivation in parentheses): percentage of adults without a high school diploma (-0.217, 0.051), percentage of adults with a college degree (0.351, 0.099), percentage of high-income households (0.324, 0.120), percentage of workers employed in a managerial or professional job (0.356, 0.120), percentage of female-headed households (0.102, 0.358), unemployment rate (0.102, 0.431), and male unemployment rate (0.132, 0.440). Although similar, these indexes measure distinct aspects of neighborhood economic circumstances. Economic opportunity captures potential social support for postsecondary education and possible economic returns to a college degree. Resource deprivation measures the extent to which adolescents are likely to grow up in a neighborhood that may lack effective social control and might not provide the economic resources that would support an adolescent in college..

Neighborhood stability measures include the percentages of housing that is owner occupied, housing that has been occupied continuously by the same households in the past five years, and residents that are foreign-born. Two Simpson Interaction Indexes (Reardon

[4] I standardize variables using the full sample of Wave II in-home respondents.

and Firebaugh 2002)[5] provide measures of racial segregation at the block group and county levels. Segregation of minority populations from white individuals occurs at varying levels. Black-white segregation tends to occur at macro-level geographies, whereas Hispanic-white and Asian-white segregation occurs at micro-level geographies (Lee et al 2008). Robust measures of segregation rely on scales at multiple levels of geography (Reardon et al 2008). Because Hispanic ethnicity is not identified uniquely from race in the census, I include the percentage of residents that are Hispanic at the block group and county levels.

Relative deprivation measures are: 1) the difference between an adolescent's GPA at Wave II and the mean high school GPA[6] at Wave I and 2) the difference between the poverty rates of the census tract in which the adolescent resides at Wave II and the block group in which the adolescent resides at Wave II. Greater values on these measure indicate that the adolescent achieved higher grades than the average at his/her high school and lives in a neighborhood that has relatively less poverty when compared with other immediate neighborhoods. To be sure, these measures do not completely capture relative deprivation. The former does not capture financial resources, whereas the latter does not capture individual-level economic deprivation. Add Health does not, however, include a parental survey at Wave II, which leaves characteristics like household income unmeasured. As a result, an individual measure of relative economic disadvantage is not possible.

[5] The Index is calculated as $100 * \sum_{r=1}^{R}[\pi_r * (1 - \pi_r)]$, where π_r represents the share of the population that is a given race. The Index ranges from 0-75 and increases as neighborhoods become more evenly divided between groups. A similar index is used by Harding (2011) to measure segregation.

[6] I use Wave I mean high school GPA because these data are unavailable at Wave II. GPA is a measure of an adolescent's grades in core academic subjects.

Measures of epidemic/peer effects are the average GPA of the adolescent's peers and whether or not the peers expect to earn a bachelor's degree. At Wave I, adolescents were asked to nominate up to five male friends and five female friends. Nominations that adolescents sent, as well as nominations they received, were used to calculate their peers' GPAs and educational expectations.

Individual mediators concurrent with residence in the disadvantaged neighborhood include high school graduation, GPA, academic mastery, future life expectations, and neighborhood attachment. High school graduation is a binary variable capturing whether an individual graduated from high school with a high school diploma; individuals completing a GED or equivalency are not included as graduates. GPA is the adolescent's academic GPA at Wave II. Academic mastery is operationalized as an adolescent's standardized score on a picture vocabulary test (PVT) at Wave I. Although this variable is temporally prior to residence in concentrated poverty at Wave II, the other measure of academic mastery in Add Health is a PVT score assessed at Wave III. At this point, the vast majority of adolescents are college aged, and academic mastery likely is affected by whether the individual attends college – leaving the possibility for reverse causality with the Wave III measure. Individual life expectations measured at Wave II include variables capturing whether or not the adolescent expects to be alive at age 35, expects to be dead by 21, expects to attend college, expects to complete a bachelor's degree, and expects to be middle class in adulthood. A neighborhood attachment/quality scale[7] (α=0.6124) measured at Wave II includes whether or not the adolescent knows most of the people in his or her neighborhood, talked with someone in his or her neighborhood, thinks people in

[7] I calculate this scale by averaging the standardized values of the component variables. I standardize variables using the full sample of Wave II in-home respondents.

the neighborhood look out for each other, feels safe in the neighborhood, is happy living in the neighborhood, and would be unhappy if forced to move from the neighborhood.

Methods

Selection effects in the relationship between concentrated poverty and collegiate outcomes confound estimation of the true effect, and incorporating the myriad variables affecting both residence in concentrated poverty and future collegiate outcomes into a single regression model would yield severely unstable estimates of the focal relationship. Propensity matching offers a potential solution to these problems. Assuming a well-specified matching algorithm, matching adolescents residing in concentrated poverty with adolescents who do not reside in concentrated poverty but are otherwise similar allows me to generate a reasonable estimate of the treatment effect of concentrated poverty.

To generate predicted probabilities of residence in concentrated poverty, I estimate a logistic regression model (the first-stage model) of residence in a block group with concentrated poverty at Wave II using the set of temporally-prior variables listed in Tables 1a and 1b. Matching variables must occur prior to the treatment or risk downwardly biasing the estimated effect. Missing values are unlikely to occur randomly, and failure to account for item non-response on the matching variables is likely to bias the matching equation (D'Agostino and Rubin 2000). Table 2 presents frequencies of missing data for the core analytic sample by residence in concentrated poverty. For the categorical variables, I treat missing values and refusals to respond as a unique category, and for continuous variables, which are mean standardized as z-scores, I replace missing values and refusals to respond with zeros and include an indicator variable indicating item non-response.

I then use a nearest-neighbor match with replacement and a caliper of 3 percent. This selects control cases that differ in predicted probability of residing in concentrated poverty from treatment cases by no more than 0.03 and allows control cases to match to multiple treatment cases. To control for school effects, I require exact matches on the community; I do allow adolescents to match with adolescents at the sister school at Wave I. I also require exact matches on race/ethnicity and sex. This procedure successfully matches 835 pairs – most adolescents are eliminated by the community requirement.

The matching equation yields a sample of matched pairs that is fairly well balanced. Tables 1a and 1b demonstrate that the full sample of Add Health respondents are imbalanced along a host of parental, adolescent, household, and neighborhood characteristics with respect to future residence in concentrated poverty. However, the analytic sample is balanced on all characteristics, suggesting any bias in estimated focal relationships due to observed covariates should be minimal. Standardized differences compare means between groups in pooled standard deviation units and are not influenced by sample size; a standardized difference of less than 0.1 is an accepted measure of balance (Austin 2011). The treatment and control groups differ in their mean predicted probabilities of residence in concentrated poverty by only 0.00085; yet, these samples differ in their mean neighborhood poverty rates by 28.4 percent. Table 3 presents summary statistics for the unweighted matched pairs prior to imputation. I account for missing data in the dependent and independent variables of the second stage models (models of college matriculation and completion outcomes) using multiple imputation (*mi ice* in Stata) with 10 imputed data sets and augmented logits to avoid perfect prediction (White et al 2010).

I estimate logistic regression models comparing the matched pairs on the college matriculation and completion outcomes (the second-stage models). Harding et al (2011) suggest effect heterogeneity – the potential for youth to get different 'doses' of a neighborhood effect based on their different individual and household characteristics – is an under-explored aspect of the neighborhood effects literature and that mechanisms of transmission also could be an important source of effect heterogeneity. Thus, in a series of stepwise models, I test mediating and moderating variables highlighted as significant by the literature. I also examine potential effect heterogeneity based on sex, race, and urban versus rural residence. Matching within community accounts for geographic clustering of the data. It also should account for some institutional factors, especially the school which is the primary institution in most adolescents' lives, that might mediate the relationship between neighborhood disadvantage and postsecondary outcomes. Wave II cross-sectional sampling weights account for differential probability of inclusion in the original Add Health analytic sample. I assign each treatment individual's sample weight to its matched control, and the second-stage models, as well as all subsequent bivariate and multivariate analyses, are weighted using sampling weights. For the weighted analyses, my sample size is further reduced by 116 pairs by requiring valid sampling weights. The remaining 719 pairs (1438 adolescents) constitute my final analytic sample.

These logistic models explicitly and completely control for one of the primary community institutions in an adolescent's life – the school – by requiring an exact match on school district. If schools are a key mechanism for neighborhood effects, controlling for school effects controls for an important type of neighborhood effect. Adelman (1999, 2006) finds that academic preparation in secondary school is the primary predictor of

whether or not an adolescent graduates from college. Student performance is an important component of secondary preparation, but far more important is curriculum intensity and quality. To test the importance of high schools as key neighborhood institutions for collegiate outcomes, I re-estimate the logistic models without requiring an exact match on school district. These models are the same as those described in the previous paragraph in every other way. This allows treatment individuals to match to control observations anywhere in the country, and less than 5 percent of adolescents residing in concentrated poverty match to a control observation that attends his/her same school or sister school. The subsequent logistic models estimating the effect of concentrated poverty on collegiate outcomes incorporate both neighborhood and school effects.

Finally, I test epidemic theory's notion that there is a threshold effect whereby passing a critical threshold of neighborhood disadvantage substantially increases the risk of negative outcomes. I re-estimate the bivariate and full models of college graduation and enrollment that control for high school effects to test all integers between 15 and 50 as potential thresholds for concentrated poverty. This yields 36 unique parameter estimates for both concentrated poverty and neighborhood opportunity (one for each threshold). I then graph the parameters of concentrated poverty (from the bivariate model) and neighborhood opportunity (from the full model) against the thresholds.

Results

Table 4 presents the frequency of postsecondary outcomes for the treatment and control groups. Over all, there are differences in college matriculation and completion based on neighborhood poverty saturation, and concentrated poverty has a larger proportional impact on the odds of graduating from college. Although both groups

graduate from college at rates below the national average, 21.6 percent of the control sample of adolescents not residing in concentrated poverty completes a bachelor's degree versus only 15.1 percent of adolescents residing in concentrated poverty. The somewhat stronger impact of concentrated poverty that appears in college completion outcomes could be indicative of a lagged effect of neighborhood disadvantage that is strongest several years after residence in the neighborhood (Sampson et al 2008). Both groups experience sharp declines from their rates of ever enrolling in college to completing a bachelor's degree. If the effect of concentrated poverty is greater on college completion than on matriculation, this could signify a doubled disadvantage of concentrated poverty – reduced educational attainment and a depressed or delayed earnings trajectory due to greater years spent in college without earning a bachelor's.

Logit models presented in Table 5 suggest that residence in concentrated poverty during adolescence has a weak, negative relationship with college matriculation. Model 1 presents the bivariate association, Model 2 includes several demographic controls, and Models 3-7 include individual-level variables, neighborhood collective socialization, neighborhood stability, relative disadvantage, and peer effects as candidate mediators/mechanisms, respectively. Model 8 tests all mediators in a pooled model. Absent contextual mechanisms (Model 2), several demographic variables demonstrate a significant relationship with an adolescent's likelihood of enrolling in college. From this relatively disadvantaged sample, women and black adolescents are more likely to enroll in college, whereas adolescents in an urban area may be less likely to enroll. These findings, including the black advantage in educational attainment once a host of covariates are controlled (Crowder and South 2011), are consistent with previous research.

Modeled separately by theory (Models 3-7), several individual and contextual factors also have a significant relationship with college enrollment. Adolescents demonstrating greater academic mastery, achieving higher GPAs, attaining high school diplomas, and expecting greater educational attainment are more likely to enroll in college. Oddly, adolescents expecting to die by age 21 are more likely to enroll in college. There are relatively few adolescents expecting to die by 21, which might yield unstable estimates. Alternatively, perhaps after controlling for several additional measures of future expectations, as well as other economic and social measures, expecting to die at a young age may capture something about an adolescent that would encourage him or her to complete life goals (e.g., go to college) quickly. Neighborhood economic opportunity is related positively to college enrollment, whereas resource deprivation has only a weak, negative association. A greater share of homes occupied by owners is positively associated with enrollment, but this relationship exists only for black adolescents. Greater neighborhood diversity yields a slight decrease in college enrollment, although the magnitude of the diversity coefficient is quite small. Other measures of neighborhood stability and composition are insignificant. Finally, adolescents who have higher GPAs relative to their schools' means, as well as those with peers achieving higher GPAs and expecting greater educational attainment, are more likely to matriculate at college.

In the pooled model (Model 8), nearly all of the previously significant contextual and individual variables remain significant with coefficients of roughly the same magnitude. The matriculation advantage of black adolescents and adolescents with higher GPAs disappears, however, as does any impact of relative academic disadvantage. Concentrated poverty's weak, negative impact on matriculation may be explained by individual

educational achievement and expectations, though the parameter estimate remains roughly consistent once these controls are included. Alternatively, neighborhood economic opportunity may explain this relationship.

The logit models presented in Table 6, which follow the same stepwise structure as those in Table 5, demonstrate the negative impact of concentrated poverty on an individual's odds of completing college. The magnitude of the impact is fairly robust, and comparing the parameter estimate across the models reveals that concentrated poverty's negative impact is quite clearly explained by neighborhood opportunity. Most associations of individual-level variables remain consistent with their relationship to college matriculation, and several of the most substantive neighborhood-level variables grow in magnitude. Notably, however, an adolescent's future expectations, as well as those of his/her peers, are not significantly related to college graduation. In addition, the percentage of homes occupied by the same household for at least five years becomes significant and may be negatively associated with graduation. This could suggest that poverty-saturated neighborhoods with low turnover amongst residents tend to entrench and magnify disadvantages that are transmitted to adolescents.

Tables 7 and 8 present the re-estimation of the logits in Tables 5 and 6 without requiring an exact match on school district, and the results indicate that high schools are *not* key institutions in terms of mediating the effect of neighborhood poverty saturation on college matriculation – although high schools may *suppress* concentrated poverty's impact on college graduation. Comparing the results of the models constrained to match on community with those unconstrained on community is an explicit test of the notion that high schools are a key institution for the transmission of neighborhood disadvantage, and

many of the key coefficients remain consistent at roughly the same magnitude. It is worth noting, however, that the estimated impact of concentrated poverty on college graduation declines in magnitude and only becomes significant once demographic controls are included in the model. To the extent that high schools do suppress the impact of concentrated poverty, the analytic sample may partially explain this effect. 'Control' individuals in the sample also have a high neighborhood poverty rate (that researchers will operationalize as poverty saturation in some cases). Perhaps, these individuals attend schools that, despite serving students from disadvantaged neighborhoods, are not adequately equipped to compensate for neighborhood poverty. By contrast, 'treatment' adolescents living in neighborhoods with over 40 percent poverty likely attend a school that has at least some experience in ameliorating the negative effects of poverty saturation. Ultimately, even in the analysis that controls for school effects, neighborhood opportunity persists as the clear mechanism by which poverty saturation might impact graduation. Further, despite the largely null findings regarding school effects, high schools are likely quite important for college outcomes; these results simply suggest that high schools may not mediate the relationship between concentrated poverty and collegiate outcomes.

Tests for threshold effects in the relationship between neighborhood disadvantage and college outcomes do not offer much support for the epidemic theory. Figure 1 indicates that operationalizing concentrated poverty between 15 and 50 percent poverty does not substantially affect the estimated relationship of college matriculation with concentrated poverty (from the bivariate model) and neighborhood economic opportunity (from the full model). The impact of concentrated poverty on matriculation may diminish past the 40 percent threshold as the control sample becomes quite disadvantaged (>20

percent poverty) in terms of neighborhood poverty as well. Figure 2 also suggests the relationship between college graduation and the focal variables does not change substantially with the changing threshold. Again, if any threshold effect exists in this analysis, it is observed at the 40 percent threshold whereby the impact of concentrated poverty declines and the impact of neighborhood economic opportunity increases. What that might suggest, then, is that once neighborhood poverty reaches a rate of roughly 20 percent, the additional impact of poverty increases becomes smaller.

Discussion

This research analyzes matched pairs of adolescents that differ in the poverty saturation of their neighborhood in an attempt to gauge the effect of residing in concentrated poverty during adolescence on collegiate outcomes. Matching on a set of adolescent, parental, household, and neighborhood selection characteristics measured prior to the focal independent variable (residing in a neighborhood concentrated with poverty) controls for observable endogeneity in the relationship between neighborhood disadvantage and collegiate outcomes. Furthermore, requiring an exact match on community controls provides a strong control for school effects and subsequently allows me to test the importance of schools by relaxing this constraint. Logistic regression models of college matriculation and completion outcomes on the set of matched pairs provide estimates of the impact of concentrated poverty. Estimates of the associations of potential mechanisms of neighborhood disadvantage with collegiate outcomes are not necessarily

causal and may not be generalizable beyond the present sample of relatively disadvantaged adolescents.[8]

Results demonstrate that residing in concentrated poverty during adolescence may somewhat reduce an adolescent's odds of enrolling in college, but poverty saturation has a clear, substantive, negative impact on an individual's odds of graduating from college. These findings indicate that the mechanisms by which neighborhoods matter for collegiate outcomes may differ over the life course. In the short term (for college matriculation), neighborhood economic opportunity may explain the impact of neighborhood poverty, but individual educational achievement and expectations also might explain poverty's effect. Individual expectations and achievement during high school do not, however, explain any of the relationship between poverty saturation and graduation. Later in the life course, neighborhood opportunity is the clear mechanism by which concentrated poverty impacts college graduation. Neighborhood opportunity increases in magnitude of importance from the matriculation models to the graduation models, suggesting that neighborhoods themselves may matter more in the long term via the economic opportunity observed and experienced by adolescents. This research finds a significant, detrimental impact of concentrated poverty while observing neighborhood characteristics at only one time period, indicating that the estimated relationships may be underestimates of the true causal effects of prolonged neighborhood disadvantage (Wodtke et al 2011). When coupled with the notion that disadvantaged neighborhoods may be more impactful during adolescence than childhood (Wodtke et al 2012), this research suggests that future

[8] This sample is drawn from a nationally-representative sample of adolescents. Although roughly 28 percent of adolescents residing in concentrated poverty are lost due to the lack of an acceptable matched control, the sample remains fairly well representative of this population. In fact, individuals in the treatment sample are somewhat less disadvantaged overall than the population of adolescents residing in concentrated poverty.

experimental and quasi-experimental designs would do well to analyze the impact of adolescent neighborhood disadvantage on college completion and possibly even later outcomes (e.g., earnings trajectories).

Whereas results do provide evidence for collective socialization mechanisms for neighborhood effects, relative advantage, epidemic, and institutional mechanisms have less support. Two measures of relative disadvantage are uncorrelated with collegiate outcomes in the full model. Peer achievement and educational expectations are associated with collegiate outcomes, but the variables do not seem to mediate the impact of concentrated poverty, suggesting that epidemic theory may not explain the mechanisms by which neighborhood economic disadvantage affects adolescents. Further, epidemic theory would suggest that neighborhood disadvantage should have a greater impact in more populous locations because the higher density of peers leads to more contagious effects of disadvantage. To that end, urban adolescents should be more susceptible to the negative effects of concentrated poverty. There is not, however, a strong relationship between urban residence during adolescence and either college matriculation or college graduation. Moreover, an interaction of concentrated poverty and urban residence is an insignificant predictor of collegiate outcomes.[9] High schools, a key neighborhood institution for adolescents, do not seem to account for the impact of concentrated poverty either, although schools may suppress the effect of severe disadvantage when compared to substantial (but not severe) disadvantage.

More extensive tests of epidemic theory examine potential threshold effects and find that they may not be as salient for neighborhood effects on collegiate outcomes as they are

[9] Results of this model are not presented here.

for other types of socioeconomic outcomes. Concentrated poverty is typically operationalized as a neighborhood of 40 percent poverty or greater (Iceland 2006; Small et al 2008), though some research uses thresholds of 20 percent or 30 percent (e.g., O'Hare and Mather 2003, Sharkey 2009). Operationalizing concentrated poverty anywhere within this range does not result in an appreciable change in its impact on college graduation, and once neighborhood poverty reaches the 20 percent threshold, additional increases may have a slightly smaller impact.

The neighborhood effects on collegiate outcomes indicate that the college process may be an important source of economic immobility in the United States. Although residing in concentrated poverty during adolescence has only a weak impact on college matriculation, it has a substantive, significant impact on college graduation. Further, although many adolescents matriculate at college, only 30-40 percent of matriculants graduate, indicating a substantial level of misplaced investment. Many adolescents are allocating high levels of time and capital without seeing the long-term financial payoff, instead experiencing the lost wages and a delayed earnings trajectory associated with postsecondary enrollment. If concentrated poverty does indeed have a stronger effect on college graduation, then individuals residing in poverty-saturated neighborhoods are both disproportionately likely to be socioeconomically disadvantaged and, given the misplaced investment in college, disproportionately likely to remain disadvantaged.

The present results suggest potential explanations for the underwhelming educational impacts of the MTO experiment (Orr et al 2003; Sanbonmatsu et al 2006). Although MTO employs the gold-standard research design, the program may be unable to estimate the effects of comprehensive neighborhood disadvantage. The program induced

minimal, fleeting changes in neighborhood poverty, and it did not affect other aspects of neighborhood disadvantage (e.g., segregation, social control, and violence). Meanwhile, MTO induced moves that uprooted individuals from their established social networks. The youth may have found it difficult to fully integrate into their new neighborhoods, and any negative impacts of acclimating to a new social environment might equal or outweigh the gains from reduced neighborhood poverty. With such challenges to analyzing neighborhood effects using a randomized experiment, non-experimental, longitudinal analysis may be the only way to adequately address neighborhood disadvantage's pernicious effects.

It remains possible that the relationships estimated here are correlational and not causal because omitted variable bias is always a concern for non-experimental designs. To the extent that any omitted variables are strongly correlated with the matching variables, their omission does not bias the results. However, if an omitted variable has an effect on both neighborhood of residence and collegiate outcomes independent of the matching variables, parameter estimates will be biased. Although this research includes an extensive set of controls as matching variables, it is impossible to definitively prove that all selection variables are included. Nevertheless, concern regarding omitted variable bias is lessened in the present case because the critical variables identified in neighborhood selection research[10] (Sampson and Sharkey 2008) and selection into high-poverty neighborhoods[11] (Quillian 2003) are included in the present propensity matching model. Another common threat to causal estimation is the time ordering question. Time ordering is not as strong of

[10] Key variables listed earlier were race, ethnicity, income, and education.

[11] Key variables listed earlier were race and living in a female-headed household.

a concern for this research as, except for a few extreme examples, college matriculation and completion outcomes are unlikely to affect an adolescent's neighborhood of residence during high school.

Future research could augment the present analysis by considering more extensive measures of relative disadvantage that may be available using other longitudinal data sets. Relative economic deprivation may be a process that operates primarily at the individual level (as is described by Jencks and Mayer 1990). If this is the case, neighborhood-level measures will not accurate capture any effects of relative disadvantage.

In addition, research could further illuminate how concentrated poverty affects adolescents by examining the proximal impacts of neighborhood opportunity on adolescents that would mediate its effect on college graduation. In preliminary analyses, I tested several candidate mediators at Wave III[12] that represent norms, collective efficacy, and social capital aspects of neighborhood collective socialization: military service (ever), civic participation, having a mentor, receiving financial support from a family member (non-spouse) or close friend, having trouble paying the bills, early childbearing (ever), incarceration or criminal activity (ever), life expectations, and changes in life expectations since late adolescence. Although nearly all are related to odds of graduation, the estimated impact of neighborhood opportunity on graduation is substantively unchanged and actually strengthens somewhat in magnitude when all potential mediators are included. Understanding how neighborhood opportunity affects college graduation would be beneficial for policymakers and researchers interested in minimizing the impact of poverty-saturated neighborhoods on individuals' life chances.

[12] Potential mediators refer to experiences at the time of survey (Wave III, ages 18-26) unless otherwise noted. This should provide some assurance regarding the temporal ordering of any relationship.

Ultimately, though, this research offers an improvement in our understanding of how neighborhoods affect individuals' collegiate outcomes. The unique features of Add Health enable the testing of several competing theoretical perspectives, and capitalizing on the study's long follow-up window, I find that neighborhoods do matter for college matriculation and graduation – under-researched but quite significant outcomes in terms of future life chances. These results offer a potential resolution of the previous non-findings from the MTO experiment and suggest that neighborhood effects are complex processes that may differ in their mechanism based on the life stage at which the outcome is measured. Further, they indicate that neighborhoods are important sources of stratification and structure their residents' opportunities for economic mobility.

Table 1a. Balance of Sample on Categorical Variables Before and After Matching

This table presents standardized differences (Std. Diff.) between individuals not living in concentrated poverty (Non CP) and those living in concentrated Poverty (CP) for categorical variables. A standardized difference of less than 0.1 is generally taken to indicate a well-balanced sample (Austin 2011). Control (non CP) observations in the matched pairs are weighted using frequency weights for the number of treatments to which the control is matched (i.e., fw=1 if control is matched to 1 treatment; fw=2 if matched to 2 treatments, etc.).

	Before Matching			**After Matching**		
	Non CP	CP	Std Diff	Non CP	CP	Std Diff
Parent Characteristics						
Non-native	16.83%	13.24%	-0.10	14.75%	14.02%	-0.02
Native	83.17%	86.76%	0.10	85.25%	85.98%	0.02
No disability	94.45%	85.39%	-0.30	88.72%	87.27%	-0.04
Disability	5.55%	14.61%	0.30	11.28%	12.73%	0.04
Less than 8th grade educ.	4.33%	6.91%	0.11	6.60%	6.20%	-0.02
No HS diploma	8.37%	17.59%	0.28	15.49%	15.15%	-0.01
GED	2.76%	4.40%	0.09	2.73%	3.75%	0.06
HS diploma	26.73%	29.21%	0.06	28.69%	30.45%	0.04
Some college	21.11%	18.01%	-0.08	15.78%	17.75%	0.05
College grad	24.15%	18.74%	-0.13	22.67%	20.78%	-0.05
Professional training	12.55%	5.13%	-0.26	8.03%	5.92%	-0.08
Adolescent Characteristics						
Male	48.75%	48.02%	-0.01	45.90%	45.90%	0.00
Female	51.25%	51.98%	0.01	54.10%	54.10%	0.00
White, non-Hispanic (NH)	56.01%	15.74%	-0.93	17.25%	17.25%	0.00
Black, NH	18.60%	63.76%	1.03	61.47%	61.47%	0.00
Asian, NH	6.92%	1.73%	-0.26	2.09%	2.09%	0.00
Other/Native American, NH	1.36%	3.65%	0.15	2.09%	2.09%	0.00
Hispanic	17.11%	15.13%	-0.05	17.11%	17.11%	0.00
1st generation immigrant	7.64%	6.39%	-0.05	6.24%	6.94%	0.03
2nd generation immigrant	14.89%	10.38%	-0.14	9.93%	11.47%	0.05
3rd generation immigrant	77.46%	83.23%	0.15	83.83%	81.59%	-0.06
Not low birth weight	90.66%	89.46%	-0.04	91.10%	89.85%	-0.04
Low birth weight	9.34%	10.54%	0.04	8.90%	10.15%	0.04
Household Characteristics						
Not receiving public assistance	78.92%	48.97%	-0.66	54.10%	54.52%	0.01

Receiving public assistance[1]	21.08%	51.03%	0.66	45.90%	45.48%	-0.01
Home language is English	89.00%	90.17%	0.04	87.34%	88.87%	0.05
Home language is not English	11.00%	9.83%	-0.04	12.66%	11.13%	-0.05
Two bio./adoptive parents in HH	54.58%	29.99%	-0.51	36.72%	32.27%	-0.09
One bio./other non-bio. parent HH	16.70%	13.88%	-0.08	13.49%	14.74%	0.04
Single-parent HH	23.00%	45.39%	0.49	40.06%	44.51%	0.09
Two step parents/Other HH	5.72%	10.74%	0.18	9.74%	8.48%	-0.04
Reason(s) for Moving into Neighborhood[2]						
Near old workplace	25.11%	19.49%	-0.14	19.07%	20.82%	0.04
Near current workplace	37.81%	24.94%	-0.28	22.45%	26.50%	0.09
Outgrown previous housing	38.63%	31.68%	-0.15	32.24%	32.02%	0.00
Affordable good housing	51.12%	52.35%	0.02	49.83%	50.86%	0.02
Less crime	62.13%	40.15%	-0.45	45.91%	42.61%	-0.07
Less illegal activity by adolescents	58.04%	37.67%	-0.42	43.51%	40.07%	-0.07
Close to friends/relatives	43.63%	44.88%	0.03	48.27%	43.90%	-0.09
Better schools	49.38%	30.74%	-0.39	33.51%	33.05%	-0.01
Children of appropriate ages	30.15%	26.32%	-0.09	25.56%	25.56%	0.00
Born here	16.35%	21.66%	0.14	24.44%	20.92%	-0.08
Primary Reason for Moving into Neighborhood[3]						
Near old workplace	4.92%	9.94%	0.19	9.58%	9.08%	-0.02
Near current workplace	1.96%	1.35%	-0.05	0.87%	1.71%	0.07
Outgrown previous housing	10.03%	6.99%	-0.11	7.67%	7.36%	-0.01
Affordable good housing	10.22%	11.04%	0.03	11.67%	11.30%	-0.01
Less crime	19.29%	23.80%	0.11	21.08%	21.92%	0.02
Less illegal activity by adolescents	12.49%	10.92%	-0.05	10.80%	10.96%	0.01
Close to friends/relatives	4.03%	3.31%	-0.04	4.70%	3.60%	-0.06
Better schools	13.67%	15.83%	0.06	12.02%	15.24%	0.09
Children of appropriate ages	16.40%	6.26%	-0.32	7.67%	7.19%	-0.02
Born here	1.24%	0.98%	-0.03	0.70%	1.20%	0.05
No-response/missing	5.75%	9.57%	0.14	13.24%	10.45%	-0.09

[1] Includes receipt of welfare/AFDC (now TANF), SSI, Food Stamps (now SNAP), unemployment or worker's compensations, and a housing subsidy or public housing.
[2] Percentage listing this as a reason for moving into the neighborhood. Dummy variables also identified those not listing this as a reason and those with missing data on the question for the matching regression.
[3] Non-response/missing individuals also did not have a response for the multiple options question above.

Table 1b. Balance of Sample on Continuous Variables Before and After Matching

This table presents standardized differences (Std. Diff.) between individuals not living in concentrated poverty (Non CP) and those living in concentrated Poverty (CP) for continuous variables. A standardized difference of less than 0.1 is generally taken to indicate a well-balanced sample (Austin 2011). These variables are mean-standardized (z-scores), and for the imputation, missing values are re-coded as zero and flagged with a missing value dummy variable. Control (non CP) observations in the matched pairs are weighted using frequency weights (fw) for the number of treatments to which the control is matched (i.e., fw=1 if control is matched to 1 treatment; fw=2 if matched to 2 treatments, etc.). Standard deviations appear in brackets.

	Before Matching			**Matched Pairs**		
	Non CP Incidence	CP Incidence	Std. Diff.	Non CP Incidence	CP Incidence	Std. Diff.
Respondent parent's age at adolescent's birth	0 [0.97]	-0.08 [1.26]	-0.08	0.04 [1.12]	-0.03 [1.12]	0.06
Household size (top coded at 15 individuals)	0.04 [0.97]	0.23 [1.28]	0.18	0.11 [1.06]	0.12 [1.21]	-0.01
Household income	0.04 [1.03]	-0.43 [0.43]	-0.55	-0.29 [0.34]	-0.28 [0.39]	-0.03

Table 2. Item Non-response or Missing Data by Question

This table presents the differences in the frequencies of missing data between individuals not living in concentrated poverty (Non CP) and those living in concentrated Poverty (CP) for the analytic sample prior to matching. Missing data do not appear to occur randomly. Missing data for a categorical variable are coded as a separate category and included in the matching model. Missing data for a continuous variable (z-scores) are mean-coded and flagged with a missing data dummy variable for each variable in the matching model.

	Non CP Missing	**CP Missing**	**Difference**
Continuous Variables			
Parent age at birth	12.35%	16.51%	4.16%
Household size	0.00%	0.00%	0.00%
Household income	22.69%	31.61%	8.92%
Parent Characteristics			
Parental nativity	12.02%	15.81%	3.79%
Parental disability	12.16%	16.11%	3.95%
Parental education	2.19%	3.24%	1.05%
Adolescent Characteristics			
Gender	0.00%	0.00%	0.00%
Race/ethnicity	0.30%	0.20%	-0.10%
Immigrant generation	1.18%	3.34%	2.16%
Low birth weight	0.00%	0.00%	0.00%
Household Characteristics			
Assistance receipt	12.22%	16.21%	3.99%
Home language	0.00%	0.00%	0.00%
Family type	0.00%	0.00%	0.00%
Reason for Neighborhood Move			
Near old workplace	13.00%	17.33%	4.33%
Near current workplace	12.97%	17.53%	4.56%
Outgrown previous housing	13.10%	18.14%	5.04%
Affordable good housing	13.27%	17.93%	4.66%
Less crime	13.41%	17.73%	4.32%
Less illegal activity by adolescents	13.93%	18.24%	4.31%

Close to friends/relatives	13.05%	16.92%	3.87%
Better schools	13.44%	17.93%	4.49%
Children of appropriate ages	13.19%	17.63%	4.44%
Born here	13.03%	17.22%	4.19%
Primary reason for move	13.34%	17.43%	4.09%

Table 3. Summary Statistics for Matched Pairs (prior to imputation)

	Obs.	Mean	Std. Dev.	Min	Max
Not residing in concentrated poverty					
College graduate	589	0.2411	0.4281	0	1
Ever enrolled in college	589	0.6112	0.4879	0	1
High school graduate	589	0.7827	0.4128	0	1
Wave II concentrated poverty	719	0	0	0	0
Wave II block group poverty rate	719	0.2231	0.1033	0	0.3996
Sex (female=1)	719	0.5410	0.4987	0	1
Black adolescent	719	0.6147	0.4870	0	1
Urban resident	719	0.4729	0.4996	0	1
PVT score	699	91.8112	14.3156	26	141
GPA	630	2.6503	0.6923	1	4
Expect to be alive at age 35	718	0.7437	0.4369	0	1
Expect to be dead at age 21	717	0.1953	0.3967	0	1
Expect to achieve middle class	716	0.4330	0.4958	0	1
Expect to earn a bachelor's degree	717	0.6722	0.4697	0	1
Expect to attend college	717	0.7225	0.4481	0	1
Neighborhood attachment	719	0.6926	0.2513	0	1
Neighborhood opportunity	322	-0.0322	0.5722	-2.2308	1.8196
Neighborhood resources	687	0.6367	0.8270	-3.6163	2.0732
% houses occupied by same resident for 5 yrs.	718	0.5604	0.1407	0.0307	0.8877
Simpson interaction index (block group)	719	29.8546	19.3598	0	72.7520
Simpson interaction index (county)	719	39.5411	13.8253	0.3368	67.3181
% Hispanic (block group)	719	0.1192	0.2436	0	0.9728
% Hispanic (county)	719	0.0981	0.1740	0.0014	0.6529
% foreign born	719	0.0839	0.1837	0	0.8998
Relative advantage (NH poverty)	719	0.1836	0.1434	-0.2771	0.6952
Relative advantage (school GPA)	466	-0.0428	0.6794	-1.8275	1.8302
Peer GPA	443	2.7270	0.5013	1.3333	3.8333
Peer expectations for bachelor's degree	447	0.1256	0.9919	-3.6320	1.1481
Residing in concentrated poverty					
College graduate	627	0.2201	0.4146	0	1
Ever enrolled in college	660	0.5879	0.4926	0	1
High school graduate	582	0.7784	0.4157	0	1
Wave II concentrated poverty	719	1	0	1	1
Wave II block group poverty rate	719	0.5064	0.0997	0.4006	0.8861
Sex (female=1)	719	0.5410	0.4987	0	1
Black adolescent	719	0.6147	0.4870	0	1
Urban resident	719	0.5090	0.5003	0	1
PVT score	686	91.5350	15.2769	10	133
GPA	625	2.6336	0.6960	1	4
Expect to be alive at age 35	715	0.7483	0.4343	0	1
Expect to be dead at age 21	712	0.2135	0.4101	0	1
Expect to achieve middle class	712	0.4326	0.4958	0	1
Expect to earn a bachelor's degree	716	0.6466	0.4783	0	1

Expect to attend college	714	0.7073	0.4553	0	1
Neighborhood attachment	719	0.6460	0.2693	0	1
Neighborhood opportunity	399	-0.2738	0.6041	-1.2538	1.7092
Neighborhood resources	637	-0.5747	1.1871	-5.8176	2.0973
% houses occupied by same resident for 5 yrs.	715	0.5132	0.1527	0	0.9360
Simpson interaction index (block group)	719	26.8783	20.0708	0	67.6885
Simpson interaction index (county)	719	39.8932	13.8323	0.3368	67.3181
% Hispanic (block group)	719	0.1403	0.2779	0	1
% Hispanic (county)	719	0.0995	0.1774	0.0013	0.6529
% foreign born	719	0.1022	0.2127	0	0.9334
Relative advantage (NH poverty)	719	-0.0997	0.1100	-0.7733	0.2498
Relative advantage (school GPA)	488	0.0067	0.6808	-1.9188	1.6222
Peer GPA	488	2.7035	0.4996	1	4
Peer expectations for bachelor's degree	486	0.0461	1.0099	-4.7351	1.1481

Table 4. Differences in Collegiate Outcomes for the Matched Pairs
(weighted and with imputed data)

	Residing in concentrated poverty	Not residing in concentrated poverty
Completed some college	50.43% [2.6%]	57.20% [2.5%]
College graduate	15.10% [1.6%]	21.59% [1.9%]

Standard errors in parentheses

Table 5. Stepwise Model of Neighborhood Disadvantage and College Enrollment
(standard errors below coefficients)

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**
Conc. Pov.	-0.2731†	-0.2668†	-0.214	0.0139	-0.2096	-0.3058	-0.2214	-0.2363
	-0.143	-0.146	-0.164	-0.18	-0.167	-0.222	-0.152	-0.265
Age		0.0296	0.0869†	0.0408	0.0166	0.0328	0.0663	0.0894†
		-0.043	-0.048	-0.044	-0.044	-0.045	-0.046	-0.053
Sex		0.5218***	0.3585*	0.5352***	0.5686***	0.4109**	0.4893**	0.3681*
		-0.145	-0.172	-0.146	-0.147	-0.152	-0.155	-0.179
Black		0.3014*	0.4287*	0.3121*	-0.157	0.4641**	0.4398**	0.1195
		-0.146	-0.185	-0.156	-0.431	-0.156	-0.166	-0.533
NH urban		-0.3896**	-0.0967	-0.3217*	-0.4026*	-0.2859†	-0.1577	-0.0849
		-0.147	-0.173	-0.148	-0.186	-0.156	-0.155	-0.234
HS diploma			1.2047***					1.1557***
			-0.239					-0.25
GPA			0.5582***					0.1844
			-0.156					-0.51
PVT score			0.0253***					0.0241**
			-0.007					-0.008
Ex alive 35			-0.2813					-0.2303
			-0.224					-0.235
Ex dead 21			0.4025†					0.49†
			-0.235					-0.251
Ex mid class			0.0617					0.0809
			-0.172					-0.177
Ex bachelors			0.6488**					0.5251*

	-0.208				-0.216
Ex college	0.5067* -0.206				0.4093† -0.212
Indiv attach. NH	-0.2773 -0.351				-0.173 -0.363
NH opportunity		0.3145** -0.107			0.2445† -0.146
NH low resources		-0.1706* -0.068			-0.1085 -0.09
NH home occ 5yr			-0.0392 -0.72		1.1342 -0.946
NH home ownership			-0.2269 -0.638		-1.1211 -0.834
Black * NH ownshp			1.1817† -0.707		1.0472 -0.857
NH Simpson block			-0.0146** -0.005		-0.0085 -0.006
NH Simpson county			-0.0043 -0.007		-0.0092 -0.009
NH Hispanic block			-0.0864 -0.737		0.1625 -1.034
NH Hispanic county			-0.3278 -0.589		0.5213 -0.775
NH foreign born			1.4972 -0.93		0.7435 -1.275
Rel. advan. (NH poverty)				-0.0392	-0.6628

						-0.56		-0.712
Rel. advan. (school GPA)						0.8777*** -0.138		0.3488 -0.494
Peer GPA							0.7321*** -0.176	0.4851† -0.274
Peer bachelor's expect.							0.2238* -0.089	0.1856† -0.11
constant	0.2903** -0.101	-0.7665 -1.234	-7.6301*** -1.662	-0.9403 -1.272	0.085 -1.412	-0.7911 -1.298	-3.9533** -1.501	-7.2543** -2.373
N	1438	1438	1438	1438	1438	1438	1438	1438

† p<0.1 * p<0.05, ** p<0.01, *** p<0.001

Table 6. Stepwise Model of Neighborhood Disadvantage and College Graduation
(standard errors below coefficients)

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**
Conc. Pov.	-0.4377**	-0.4508**	-0.5456**	-0.009	-0.2509	-0.1171	-0.4291*	-0.1052
	-0.167	-0.171	-0.196	-0.228	-0.193	-0.274	-0.179	-0.382
Age		-0.0283	0.0507	-0.0044	-0.0218	-0.0136	0.016	0.0743
		-0.052	-0.057	-0.054	-0.054	-0.053	-0.055	-0.064
Sex		0.6719***	0.5278*	0.7102***	0.6941***	0.5109**	0.6532***	0.5576*
		-0.185	-0.221	-0.189	-0.185	-0.194	-0.196	-0.236
Black		0.8000***	1.4007***	0.7619***	-0.145	1.0054***	1.1007***	0.4962
		-0.195	-0.221	-0.2	-0.544	-0.202	-0.227	-0.663
NH urban		-0.3357†	-0.1986	-0.2086	-0.1184	-0.2332	-0.063	-0.0006
		-0.178	-0.222	-0.19	-0.228	-0.19	-0.19	-0.324
HS diploma			1.6193***					1.5437**
			-0.486					-0.518
GPA			0.8737***					0.3726
			-0.173					-0.852
PVT score			0.0485***					0.0447***
			-0.009					-0.01
Ex alive 35			0.1371					0.1689
			-0.251					-0.272
Ex dead 21			-0.2505					-0.0747
			-0.306					-0.313
Ex mid class			0.2944					0.2168
			-0.209					-0.232
Ex bachelors			0.5024					0.4763

	-0.335				-0.318
Ex college	0.318				0.3397
	-0.367				-0.357
Indiv attach. NH	-0.8992*				-0.5907
	-0.435				-0.45
NH opportunity		0.6736***			0.5497***
		-0.113			-0.153
NH low resources		-0.2552**			-0.0429
		-0.09			-0.118
NH home occ 5yr			-1.9513*		0.0971
			-0.806		-1.051
NH home ownership			0.748		-0.371
			-0.834		-1.16
Black * NH ownshp			1.401†		1.6437
			-0.849		-1.052
NH Simpson block			-0.0062		0.0097
			-0.005		-0.006
NH Simpson county			0.0231*		0.0088
			-0.01		-0.011
NH Hispanic block			-1.3977		0.5289
			-1.265		-1.132
NH Hispanic county			-0.8271		0.143
			-0.897		-1.152
NH foreign born			2.0329		0.0705
			-1.511		-1.54
Rel. advan. (NH poverty)				1.4154*	0.6174

						-0.721		-0.897
Rel. advan. (school GPA)						1.1414***		0.4716
						-0.147		-0.804
Peer GPA							1.1616***	1.0080*
							-0.256	-0.403
Peer bachelor's expect.							0.2557*	0.0971
							-0.118	-0.149
constant	-1.290***	-1.1971	-12.347***	-1.6463	-1.5235	-1.9475	-6.0226**	-14.925***
	-0.114	-1.508	-2.238	-1.551	-1.705	-1.511	-1.934	-3.257
N	1438	1438	1438	1438	1438	1438	1438	1438

† p<0.1 * p<0.05, ** p<0.01, *** p<0.001

Table 7. Results of Logit Model of College Enrollment without School District Match
(standard errors below coefficients)

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**
Conc. Pov.	-0.2557†	-0.2475	-0.2833	0.1666	-0.1539	-0.0906	-0.3576*	-0.3263
	-0.149	-0.155	-0.183	-0.191	-0.185	-0.247	-0.163	-0.296
Age		0.0227	0.1013†	0.0238	0.0244	0.0164	0.047	0.1195*
		-0.043	-0.055	-0.044	-0.043	-0.047	-0.046	-0.058
Sex		0.5596***	0.4079*	0.6140***	0.5522***	0.4694**	0.5992***	0.4639*
		-0.143	-0.18	-0.145	-0.145	-0.152	-0.151	-0.187
Black		0.3953*	0.4012*	0.3753*	1.1928**	0.4302**	0.4356**	0.8349
		-0.154	-0.196	-0.165	-0.421	-0.166	-0.165	-0.508
NH urban		0.0968	0.1585	0.0759	0.0112	0.0956	0.2643	-0.2729
		-0.154	-0.183	-0.159	-0.183	-0.159	-0.169	-0.241
HS diploma			1.3914***					1.3586***
			-0.202					-0.223
GPA			0.4675***					1.1681*
			-0.131					-0.493
PVT score			0.0298***					0.0308***
			-0.006					-0.007
Ex alive 35			-0.3872†					-0.3557
			-0.234					-0.245
Ex dead 21			-0.1307					-0.1221
			-0.253					-0.266
Ex mid class			-0.1					-0.1808
			-0.18					-0.187
Ex bachelors			0.8239***					0.8005***

	-0.215				-0.214
Ex college	0.4066† -0.232				0.3071 -0.233
Indiv attach. NH	-0.9745** -0.37				-0.7674† -0.402
NH opportunity		0.5760*** -0.115			0.5107*** -0.145
NH low resources		-0.1620** -0.062			-0.086 -0.087
NH home occ 5yr			0.0652 -0.658		1.3127 -0.933
NH home ownership			0.7523 -0.633		-1.1975 -0.796
Black * NH ownshp			-1.0376 -0.647		-0.1836 -0.81
NH Simpson block			0.0005 -0.004		0.0045 -0.005
NH Simpson county			-0.0038 -0.006		-0.0115 -0.008
NH Hispanic block			-0.5712 -0.659		-0.4663 -0.78
NH Hispanic county			0.4711 -0.659		1.2689 -0.87
NH foreign born			2.1024* -0.842		2.5915* -1.092
Rel. advan. (NH poverty)				0.6867	-0.586

						-0.568		-0.72
Rel. advan. (school GPA)						0.7550***		-0.6119
						-0.113		-0.477
Peer GPA							0.6808***	0.1253
							-0.176	-0.281
Peer bachelor's expect.							0.2547*	0.1216
							-0.102	-0.14
constant	0.2866**	-0.9063	-7.7186***	-0.7586	-1.5708	-0.6761	-3.3976*	-10.229***
	-0.104	-1.24	-1.764	-1.263	-1.336	-1.358	-1.329	-2.304
N	1438	1438	1438	1438	1438	1438	1438	1438

† p<0.1 * p<0.05, ** p<0.01, *** p<0.001

Table 8. Results of Logit Model of College Graduation without School District Match
(standard errors below coefficients)

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**	**Model 8**
Conc. Pov.	-0.2809	-0.3083†	-0.4088†	0.2239	-0.2477	0.2287	-0.5492**	-0.3903
	-0.173	-0.178	-0.233	-0.234	-0.231	-0.304	-0.2	-0.429
Age		-0.0091	0.0664	-0.0276	-0.0091	-0.0353	0.0298	0.0974
		-0.054	-0.057	-0.055	-0.054	-0.055	-0.06	-0.067
Sex		0.7308***	0.5556*	0.8640***	0.8034***	0.6062**	0.8464***	0.7133**
		-0.19	-0.232	-0.2	-0.202	-0.201	-0.204	-0.257
Black		0.2938	0.6376**	0.1903	0.6226	0.3596†	0.2723	0.146
		-0.185	-0.238	-0.187	-0.551	-0.201	-0.213	-0.621
NH urban		-0.1358	-0.24	-0.2514	-0.0397	-0.2344	0.0838	-0.3155
		-0.18	-0.224	-0.195	-0.226	-0.2	-0.193	-0.285
HS diploma			1.6627**					1.7673**
			-0.558					-0.613
GPA			0.8924***					1.2044†
			-0.16					-0.626
PVT score			0.0450***					0.0368***
			-0.007					-0.009
Ex alive 35			0.436					0.237
			-0.3					-0.326
Ex dead 21			-0.1512					-0.2544
			-0.349					-0.372
Ex mid class			0.1607					0.2804
			-0.225					-0.246
Ex bachelors			0.9032*					0.7934*

	-0.369				-0.399
Ex college	0.53				0.5413
	-0.441				-0.477
Indiv attach. NH	-1.3755***				-1.4578***
	-0.37				-0.397
NH opportunity		0.8551***			0.6296***
		-0.115			-0.159
NH low resources		-0.1966*			-0.1399
		-0.081			-0.109
NH home occ 5yr			-1.267†		1.0846
			-0.747		-0.966
NH home ownership			0.9962		-1.6184
			-0.868		-1.089
Black * NH ownshp			-0.2903		0.8006
			-0.85		-1.016
NH Simpson block			-0.0208***		-0.0198**
			-0.005		-0.007
NH Simpson county			0.0057		-0.002
			-0.008		-0.011
NH Hispanic block			-0.886		0.612
			-1.127		-1.066
NH Hispanic county			0.0506		0.15
			-0.929		-1.639
NH foreign born			1.6172		0.6644
			-1.405		-1.447
Rel. advan. (NH poverty)				1.8949*	0.3195

						-0.794		-0.93
Rel. advan. (school GPA)						1.0762***		-0.2906
						-0.156		-0.642
Peer GPA							1.0226***	0.2529
							-0.215	-0.312
Peer bachelor's expect.							0.6032***	0.4982**
							-0.118	-0.162
constant	-1.411***	-1.6376	-12.549***	-0.9438	-1.467	-1.2214	-5.6179**	-13.127***
	-0.122	-1.52	-2.149	-1.536	-1.768	-1.509	-1.899	-3.298
N	1438	1438	1438	1438	1438	1438	1438	1438

† p<0.1 * p<0.05, ** p<0.01, *** p<0.001

Figure 1. Logit Coefficients for Relationship between Key Parameters and College Enrollment by Thresholds for Concentrated Poverty (from the within-community match models)

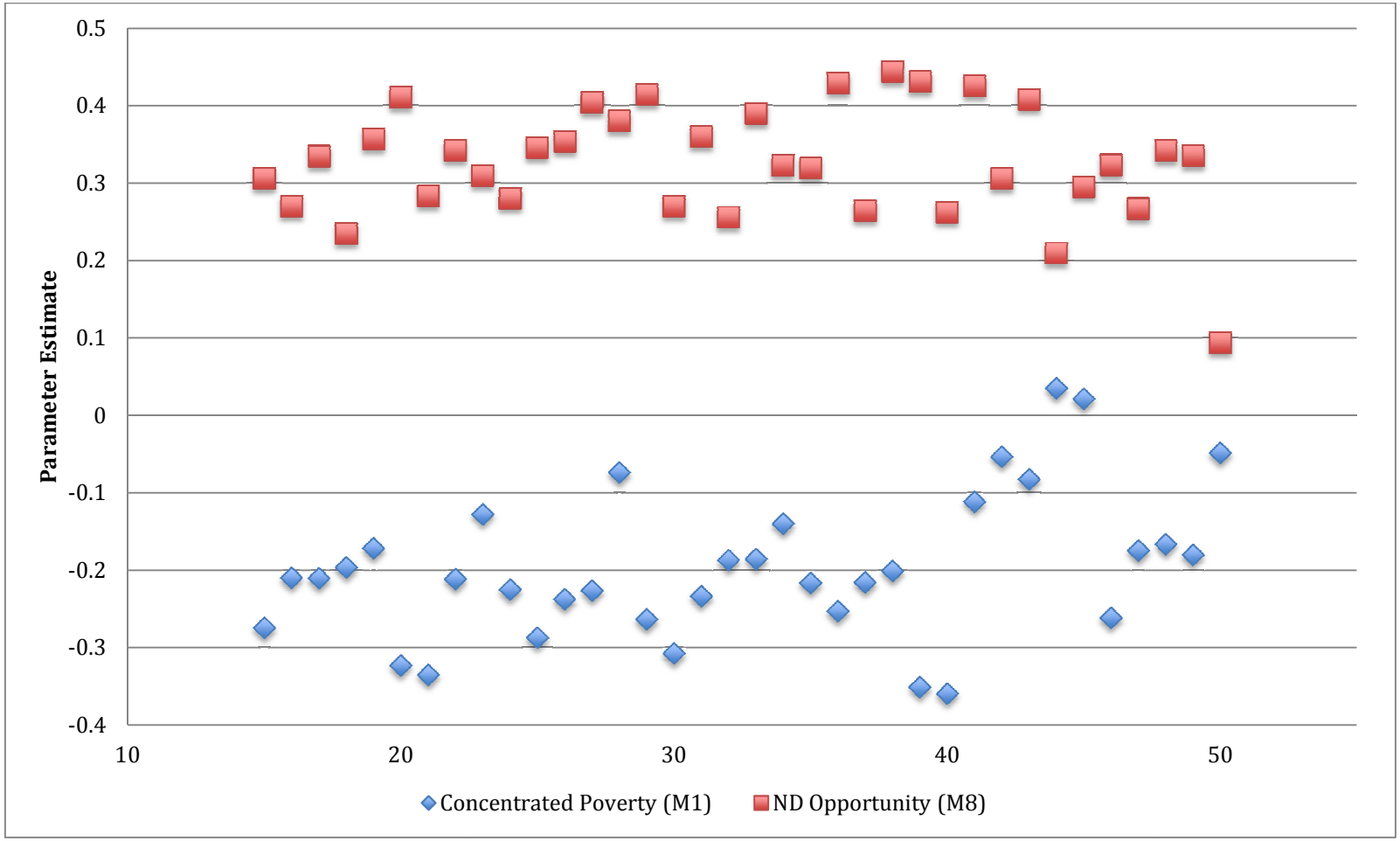
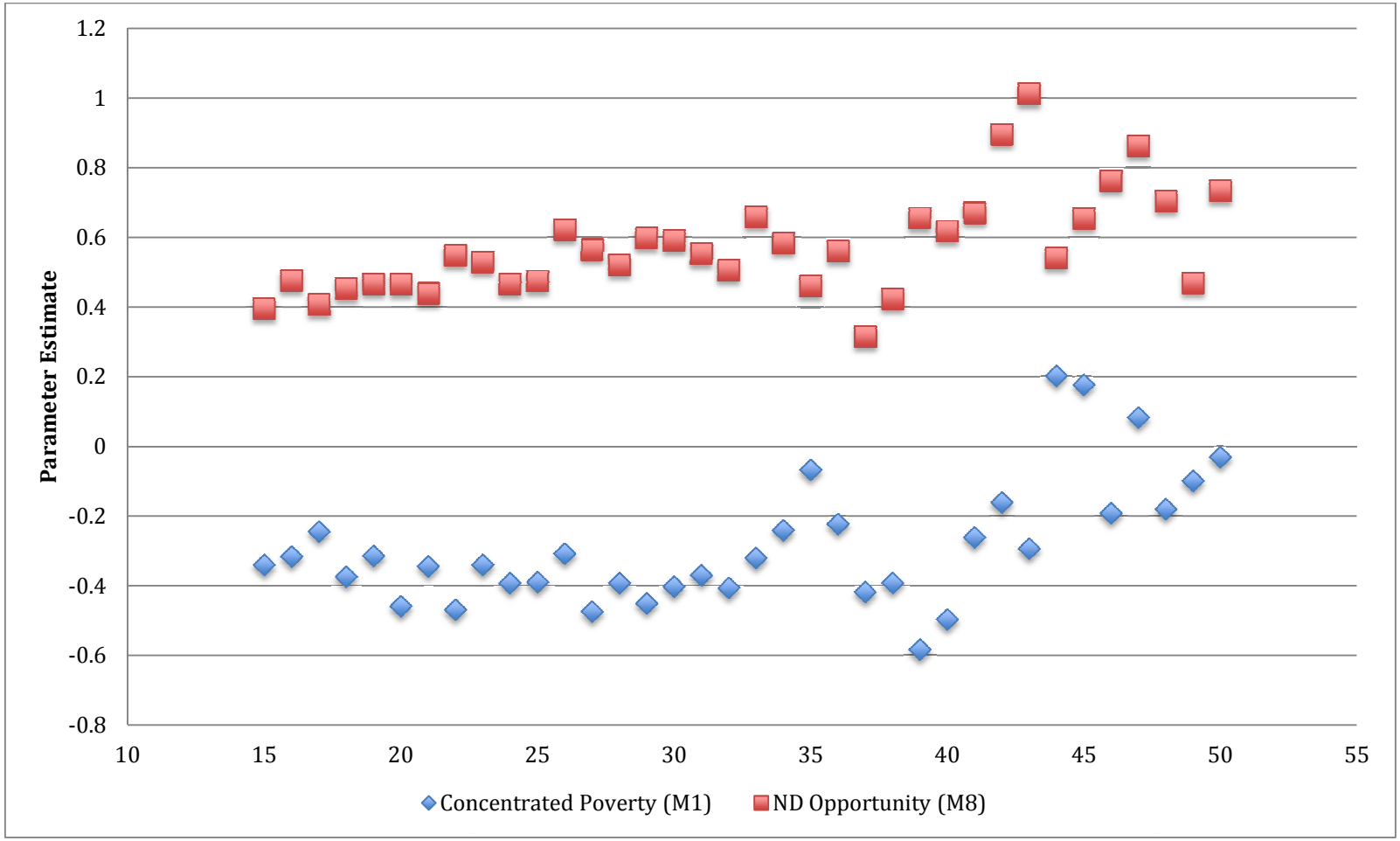


Figure 2. Logit Coefficients for Relationship between Key Parameters and College Graduation by Thresholds for Concentrated Poverty (from the within-community match models)



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